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THE IMPORTANCE OF FRENCH TRANSFORMIST IDEAS FOR THE SECOND VOLUME OF LYELL'S PRINCIPLES OF GEOLOGY

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RECENTLY there has been considerable revaluation of the development of natural sciences in the early nineteenth century, dealing among other things with the works and ideas of Charles Lyell. The task of interpreting Lyell in balanced terms is extremely complex because his activities covered many fields of research, and because his views have been unwarrantably distorted in order to make him the precursor of various modern scientific positions. Martin Rudwick in particular has contributed several papers relating to Lyell's *Principles of geology*, and has repeatedly stressed the need for a comprehensive evaluation of Lyell's scientific proposals, and of his position in the culture of his time. (1) In the present paper I hope to contribute to the reassessment of Lyell's work by concentrating on his discussion of transformism, which constituted the central theme of the second volume of the *Principles of geology* : the very length of Lyell's detailed and critical analysis of Lamarck's theories reveals the importance he attributed to the question of transformism in the contemporary natural sciences. (2)

Several authors have analyzed major aspects of Lyell's work in relation to contemporary debates in the biological and geological sciences, but no one has yet paid sufficient attention to the question of Lamarckism, or to theories and attitudes sympathetic to Lamarck's ideas in the 1820's and early 1830's, or to Lyell's awareness of the renewed circulation of transformist ideas. Furthermore, a careful if not exhaustive inquiry into the French biological thought of these years reveals that the " Âge de Cuvier" was more complex than historians of science have tended to admit.

In the preface to the third volume of the *Principles*, Lyell informed the reader that after the publication of the first volume, in January 1830, he applied himself "to perfect what I had written on the Changes in the organic world - a subject which merely occupied four or five chapters in my original sketch, but which was now expanded into a small treatise". (3) Between the initial plan - to write four or five chapters on the changes in the organic world - and the final result of his work on this subject - the first eleven chapters of the second volume - Lyell made two long geological expeditions to the continent. The first journey lasted from May 1828 to the beginning of March 1829 ; the second occupied the summer of 1830. (4)

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In September of 1830 Lyell spent six weeks working in the private museum of Gérard Paul Deshayes (1797-1875), one of the best contemporary conchologists. In important ways, the scientific results of these journeys determined the character of the *Principles of geology* : his geological observations convinced Lyell that it was possible to produce a detailed formulation of what has been characterized as his uniformitarian and "steady-state" model of explanation of geological phenomena ; his renewed acquaintance with many leading French naturalists, and the fertile discussions he had with them, reinforced his awareness of the degree to which geological questions were integrally related to the most fundamental problems of natural history. This paper will emphasize the extent to which Lyell's intimacy with the latest developments in the natural sciences in France permeated those chapters of the second volume of the Principles concerned with the question of species.

I

The theme which contributed the implicit polemical centre of Lyell's second volume was Lamarck's theory of the transmutation of species, as formulated both in the *Philosophie zoologique* and in the *Histoire naturelle des animaux sans vertèbres.* (5)

It is generally believed that in the early nineteenth century Lamarck's name was primarily, and negatively, linked with his transformist hypothesis. It is true that Lamarck's transformist ideas aroused considerable opposition, and contributed to his considerable - but not at all total - isolation in the French scientific world. But his botanical work and his several memoirs on conchology which were published in the *Annales du Muséum* and other scientific journals, together with his *Histoire naturelle des animaux sans vertèbres*, published between 1815 and 1822, won general acceptance among European naturalists. Lamarck's influence, which contributed to the abandonment of the Linnaean system of classification in the group of animals Lamarck called “invertebrates”, was also felt in Britain. The works of Samuel Brooke (1815), of the Sowerby family (1820-34), and of W. Turner (1822), successfully introduced Lamarck's taxonomic proposals to English naturalists.

The Scottish naturalist John Fleming (1785-1857), whom Lyell greatly admired, published his *Philosophy of zoology, or a general view of the structure, function and classification of animals* in 1822. Fleming's avowed intention was to give further impulse to a vitalistic theory of life, in support of the *Inquiry into the opinions, ancient and modern, concerning life and organization* (1822) by John Barclay (1758-1826). Fleming opposed materialistic tendencies which had appeared in the biological and physiological sciences, believing in a “vital principle” inherent in the embryo or germ, having the power of “developing in succession the destined plan of existence”. By means of this power, the germ was able to attract particles

of inanimate matter, and “bestow on them an arrangement widely different from that which the laws of chemistry and mechanics would have assigned them”. (6) He then characterized the differences between animate and inorganic creation, and discussed Lamarck's failure to recognize the action of irritability in plants :

> Mr Lamarck in his introduction to his valuable *Histoire naturelle des animaux sans vertèbres*… refers some of the movements which are here considered as indicating the existence of irritability in plants, to the influence of the mechanical or chemical powers, and others, to what he terms “vital orgasms”. All these different actions, however, occur in continuation with the vital principle, and their entire dependence on the laws of inorganic matter is a gratuitous assumption. (7)

Although Fleming professed the strongest opposition to the “materialistic” tenets of Lamarck, even a cursory reading of his book reveals that he followed in detail many of Lamarck's ideas. He fully accepted Lamarck's binary system of classification and recognized the validity of a taxonomic model based on the development of the nervous system, an idea developed by Julien Joseph Virey (1775-1846) and Georges Cuvier (1769-1832), and consistently applied (with obviously different overtones) by Lamarck. On several occasions Fleming expressed his admiration for Lamarck, who had overcome the difficulties facing such systems of classification, especially in the case of those many classes of animals characterized by a diffuse nervous system. Nevertheless, in following Lamarck's methods the Scottish naturalist carefully dissociated himself from other aspects of Lamarck's theory relating to the development of the nervous system.

> It is not our intention to occupy the time of the reader in a refutation of an author who, in his delineation of the mental powers of animals, substitutes conjectures for facts, and speculations for philosophical induction. Fortunately for his reputation, he possesses much real merit as a systematic naturalist. (8)

Eight years later, in the midst of his Tierce polemic with William Sharp MacLeay (1792-1856) about the latter's quinary system, Fleming repeated his praise of Lamarck, the defender of the system of progressive development who "has greatly excelled all his predecessors, in the number of his examples, and the freedom of the announcements". Nevertheless, "all the scheme is a dream of imagination", since geological remains testified against Lamarck's theory. (9) Fleming's opponent, MacLeay, whom Fleming accused of having absorbed many of his ideas from Lamarck, answered by repeating the view of Lamarck which he had expressed in his *Horae entomologicae* (1819) ; it is noteworthy that MacLeay believed in 1819 that Lamarckism had no followers in England :

His peculiar and very singular opinions have never gained many converts in his own country and I believe none in this. They are indeed only to be understood by those who are already supplied with the means of refuting them, so that the mischief they may have occasioned being comparatively

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null, we may be permitted to assign due praise to the labours of Lamarck, as being one of the first zoologists France has produced ; as being those of a person, whose merit in natural history bear much the same relation to those of Cuvier, that the world has been commonly accustomed to institute between the calculations of the theoretical and the observations of the practical astronomer. (10)

Fleming had good reason to believe that this conviction about the lack of influence of Lamarck could no longer be sustained. He was well acquainted with the Lamarckian Robert Edmund Grant, and was probably aware that materialistic and progressionist ideas were gaining force among Edinburgh students, and were fascinating Lyell himself who had expressed a certain admiration for Lamarck's work. In October 1826, Grant had anonymously published a paper entitled "Observations on the nature and importance of geology", in the *Edinburgh new philosophical journal*. Grant, who was in 1827 to become the first professor of comparative anatomy and zoology at University College London, expressed his deep admiration even for Lamarck's more controversial theories. Grant maintained that fossil discoveries were increasingly tending to support Lamarck's hypothesis, though he too recognized that

this meritorious philosopher… has resigned himself to the influence of imagination, and attempted explanations, which, from the present state of our knowledge, we are incapable of giving ; nevertheless we feel ourselves drawn towards it [i.e. Lamarck's system], and the notions of the progressive formation of the organic world, must be found more worthy of its First Great Author, than the limited conceptions that we commonly entertain. (11)

It is possible that Lyell had become acquainted with Lamarck's ideas before 1827, during his various journeys to France, or, even more likely, through the many discussions on natural history and geology he had with Fleming and Gideon Mantell (1790-1852), the fellow naturalist who actually gave Lyell his copy of the *Philosophie zoologique* to read. (12) What is certain is that Lyell read Lamarck's work in February 1827, some months after the defence of Lamarck published in the *Edinburgh new philosophical journal*. The reading of Lamarck occupied a crucial place in the development of Lyell's ideas. His review of the *Transactions of the Geological Society*, published in the *Quarterly review* in 1826, argued for a progressive and ascending scale in the succession of the forms of life on the surface of the earth ; even the anatomical characteristics of man's constitution had their own place in the progressionist model ; (13) the *Principles of geology* took an opposite line, and strongly opposed progressionism : man's recent entry to the stage of life was seen as unrelated to any plan of increasingly complex organization which during the course of ages achieved its most perfect result in the constitution of man.

This undoubtedly radical change of attitudes has been interpreted in different ways. (14) The most recent and more organic reassessment of this

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important aspect of Lyell's intellectual life, centred upon the reading of Lamarck, has been given by Michael Bartholomew. A famous letter written by Lyell to Mantell referring to his impressions of

Lamarck's book, and several passages of his Journals noted by L. G. Wilson, give some idea of how important to his intellectual development Lyell himself considered his reading of Lamarck. As Bartholomew has perceptively pointed out, the more Lyell reflected on Lamarck's theories, which had fascinated him from the first, the fuller was his realization of their implications. (15) A letter written thirty-six years later gives a hint of Lyell's afterthought :

I remember it was the conclusion he came to about man that fortified me thirty years ago against the great impression which his arguments at first made on my mind… When I came to the conclusion that after all Lamarck was going to be shown to be right, that we must "go the whole orang", I re-read his book, and remembering when it was written, I felt I had done him injustice. (16)

It therefore seems likely that it was the materialistic overtones of the transformist hypothesis with respect to human physiology and psychology that determined Lyell's strong reaction against Lamarck. Indeed, Lyell made a great effort to show the impossibility of a graduated scale of intelligence, of mental and intellectual faculties, front the less organized beings up to man. It is obvious, and was the more so to his contemporaries, that Lyell's arguments were not only opposed to a progressionist and transformist approach, but were also inimical to the idea that man's moral and intellectual prerogatives could be explained in terms of the high degree of development of physiological structures.

The materialism of Lamarck's theories was widely discussed, for instance in the debate between Fleming and MacLeay mentioned above. A particularly important attack on materialism from the point of view of the natural sciences came from the authoritative pen of Sir Humphry Davy (1788-1829) who emphasized, in his widely read Consolation in travel, or the last days of a philosopher (1830), the materialistic dangers of transmutationist ideas. Although Lamarck was not mentioned by name, it is clear that Davy was drawing upon the ideas of Lamarck and De Maillet. Onuphrio, one of the characters of the third dialogue, "The unknown", states :

I will not support the sophisms of that school which supposes that living nature has undergone gradual changes by the effect of its irritability and appetencies ; that the fish has in millions of generations ripened into the quadruped, and the quadruped into the man ; nor that the system of life, by its own inherent power, has fitted itself to the physical changes in the system of the universe. To this absurd, vague, atheistical doctrine, I prefer even the dream of plastic powers… (17)

Davy himself, in the same third dialogue of his *Consolation*, presented through the voice of the Unknown a progressionist and directionalist

model of interpretation for the succession of life on earth. Lyell, aware of how Davy's opinions weighed with his contemporaries, criticized this scientist's views in chapter IX of the first volume of the *Principles.*

Davy was aware of the possible materialistic uses of a progressionist model. Philalethes, in the fourth dialogue, strongly attacked the progressionist interpretation of the development of the nervous system on the grounds of its atheistical overtones. In an implicit reference to the lectures delivered by William Lawrence at the Royal College of Surgeons, and publislied in 1819, Philalethes remarked :

When I heard with disgust, in the dissecting room, the plan of the physiologist, of the gradual accretion of matter, and its becoming endowed with irritability, ripening into sensibility, and acquiring such organs as were necessary, by its own inherent forces, and at last rising into intellectual existence, a walk into the green fields or woods by the banks of rivers brought back my feelings from Nature to God. (18)

Lyell was not insensible to the writings of the political economists, and to the general cultural climate in which more and more stress was put on the possibility (and ideological necessity) of explaining natural phenomena in terms of "secondary causes", or necessary laws ; he was not, therefore, sympathetic towards an approach which tended to place more than the due emphasis on the providential element in the

government of nature. But nor was he willing to accept a scientific hypothesis which explicitly favoured materialistic conclusions. He thought that it was possible to implement a fully scientific approach to the natural sciences without affronting the prejudices of the age and his own beliefs. We shall see how lie was convinced that he had elaborated an acceptable natural explanation of the succession of species through geological epochs, and how at the same time he thought he had avoided any link with Lamarck, presenting himself as the definitive critic of transformism.

Another event which can help our understanding of Lyell's attitude towards Lamarck was the publication in 1826 of J. C. Prichard's (1786-1848) second edition of his *Researches into the physical history of mankind*. (19) Prichard, following Ray, Buffon and Blumenbach, defined species as groups of individuals which could interbreed, and produce offspring capable of perpetuating their type. Species modified themselves into varieties according to differences of circumstances, although the extent of the variability was thought to be very limited. Prichard also doubted whether such variations could be explained in terms of the direct action of the environment. The physiological character of species, the possibility of procreation, never altered. Prichard admitted that several species exhibited an impressive number of morphological varieties, as in the case of the dog, of the horse, and of man himself. But no degree of diversity placed in question the unity of these different varieties, or of their being only morphological modifications of the same species.

Lyell was impressed by Prichard's careful examination of the question of variability, and by his vast erudition in every branch of the natural sciences ; he was to use Prichard's text as a mine of information and quotations - Prichard is without doubt the most quoted author in the second volume of the *Principles*. In sum, Prichard's detailed inquiries into the phenomena of variation, and his strong conviction of the fixity of species, probably gave Lyell further stimulus to enter the field of the debate on transformism. For Lyell, Lamarck's theory became another example - though a fascinating one - of the consequences of using “imagination” in the natural sciences, and of substituting cosmology for inquiry into the actual processes of nature.

It is significant that Lyell's change of attitude between 1826 and 1832 with respect to progressionist models occurred over the same period as the two journeys which he made to France and Italy. During these journeys through the French and the Italian countryside, from Auvergne to Sicily, Lyell made large collections of extant and fossil shells. He discussed the problem of their exact classification with Jules Pierre Desnoyers (1800-1887), who in 1831 became the Secretary of the French Geological Society. Desnoyers had published a paper in the *Annales des sciences naturelles* in which he illustrated the characteristics of tertiary formations throughout Europe. He emphasized the possibility of considering the tertiary formation as a distinct one, characterized by a succession of different basins formed through the action of various incursions and withdrawals of the sea, and through the elevation and subsidence of the levels caused by the action of volcanic forces. (20) Desnoyers lectured on the subject at the Geological Society of Paris ; Deshayes was in the audience, and informed Desnoyers that through the analysis of his personal collection of shells he had reached the same conclusions as far as the succession of basins in tertiary formations was concerned. Desnoyers directed Lyell to Deshayes, and Lyell took great advantage of this introduction. He used Deshayes's abilities to determine the specific character of the shells which he had collected or had received as presents from various Italian naturalists.

A further interesting aspect of this cooperation is that Deshayes had been closely associated with Lamarck ; he does not seem to have accepted every aspect of Lamarck's philosophy of nature, but he always defended the reputation of his teacher. From 1833, he joined Henry Milne Edwards (1800-1885) in the republication of the *Animaux sans vertèbres*, and he had written the article “Conchyliologie” in the *Dictionnaire classique des sciences naturelles*, where he expressed his great admiration for Lamarck's work on the classification of the molluscs ; this showed “cet ésprit de philosophie analytique qui

caractérise tout ce qui sort de sa plume sévère". In the article "Mollusque" of the same *Dictionnaire*, Deshayes emphasized how

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Lamarck's system of classification as elaborated in *Animaux sans vertèbres* had, though not perfect in every point, contributed to the overthrow of the systems proposed by some dogmatic followers of Linnaeus. (21)

In the course of six weeks spent with Deshayes, Lyell doubtless often heard his French colleague speak well of Lamarck, whom the French conchologist had called "le Linnée Français". Lyell was again favourably impressed by Lamarck's abilities, though he subjected transformism by this stage to the kind of pungent criticism which was to characterize his assessment of Lamarck's theories in the *Principles*. He wrote ironically to his sister, commenting upon the enormously long span of life of a form of shell, that "it must therefore have required good time for Ourang-Outang to become man on Lamarckian principles". (22)

Deshayes had been a close collaborator of another French naturalist, André Étienne d'Auberard de Férussac (1786- 1836), whom he helped in his work on the molluscs. (23) The relationship between the two became more and more difficult mainly because of the character of Férussac ; nevertheless, after the death of the baron, in January 1836, Deshayes completed the latter's unfinished work. Some years before, during one of his previous journeys to France, Lyell had met Férussac, (24) who discussed with him his speculations about the gradual contraction of a primeval ocean, which made possible the successive formation of the different strata and the creation of primitive forms of life. Apart from his concept of a primeval ocean, and his support for the hypothesis of a progressive cooling down of the globe, Férussac maintained that universal "bouleversements" were pure imagination, and that the only revolutions suffered by the earth's surface had always been of a local character. (25) Férussac, like Deshayes, was one of the major contributors to the *Dictionnaire classique* mentioned above. Among other articles, he wrote on the geographical distribution of the molluscs, where he proposed a theory of centres of creation which will be discussed below. Férussac was a friend of Jean Baptiste Bory de Saint Vincent (1780-1846), the editor of the *Dictionnaire*, and the most influential and outspoken of the naturalists inspired by Lamarck's work. To illustrate the importance of the *Dictionnaire*, it needs only to be pointed out that Alexandre Brongniart (1770-1847), the two De Candolles, Louis Constant Prévost (1781-1856), Etienne Geoffroy Saint Hilaire (1772-1844), his son Isidore Geoffroy Saint Hilaire (1805-1861), H. Milne Edwards, Karl Sigmund Kunth (1788-1850), Pierre André Latreille (1762-1833) and Achille Richard (1794-1859) were among its contributors. It is therefore arguable that either Deshayes or Férussac would surely have familiarized Lyell with the most recent developments of French natural sciences. Furthermore, Lyell quoted the papers contributed to the Dictionnaire by both Férussac and Deshayes ; Férussac's contain an explicit invitation to the reader to consult Bory's article "Création".

Bory de Saint Vincent contributed several leading articles to this

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Dictionnaire : particularly interesting are the articles "Homme", "Instinct", "Intelligence". (26) In the articles "Histoire naturelle", "Matière", "Création", and "Géographie sous le rapport de l'histoire naturelle", Bory revived an explicitly materialistic system of natural philosophy, firmly grounded on Lamarck's ideas and on the tradition of French eighteenth-century materialism. (27) Bory repeatedly eulogized Lamarck, and shared Deshayes's conviction that the work of their older colleague was the French equivalent of that of Linnaeus. (28) Bory maintained that, if matter itself had not the faculty of thought, "la pensée, étant un effet nécessaire d'un certain ordre d'organisation, dès que cet ordre se trouve établi, la pensée en dérive nécessairement". Bory also maintained that matter, organic and inorganic, was a kind of universal substance, which appeared in différent "états". He described at great length the six

fundamental “états” : “mouqueux, vésiculaire, agissant, végétatif, cristallin, et terreux”. He explained the formation of life by the aggregation of some of the said kinds or status of matter : at the lowest stage of life were molecules of living matter ; a mechanism of expansion and contraction of molecules of air ; or of some gases, enveloped by this nucleus of living matter, was the basis for further developments from infusoria to the highest degrees of organization. (29)

It is not surprising that Bory praised Lamarck's use of the concept of irritability, and his theory of “orgasm”. He also maintained that the first appearance of a level of organization was instantly accompanied by the appearance of instinct : “à mesure que l'être organisé s'élève en complication, et que des sens se viennent cumuler chez lui, ces effets constants et saillans qui résultent de la combinaison de peu d'organes vitaux se fondent, pour ainsi dire, dans des nouvelles facultés où le nombre apporte des modifications non moins admirables par leur effets”. (30) Bory de Saint Vincent differed in some important points from Lamarck ; for instance he carefully avoided in his system any reference to Lamarck's ideas on the tendency to higher degrees of organization. He pointed out that growth could be explained as a universal mechanical principle of successive aggregations, which had the same status as the law of gravitation.

Lyell was aware that Deshayes's and Bory's tolerance towards Lamarck was not an isolated phenomenon. Etienne Geoffroy Saint Hilaire advised young naturalists to read Lamarck, and indicated that the latter's discussion of the concept of use and disuse of organs was one of the best chapters ever written in natural history on the influence of external circumstances. (31) Geoffroy's admiration for Lamarck was also, at least in part, an aspect of his struggle against Cuvier. He accepted some elements of Lamarck's theory, though he translated the points of agreement into the language of his own system.

Geoffroy recognized, following the classic definition of Buffon, that species are characterized by their capability of reproducing the same morphological and physiological type. In the process of reproduction, a

nisus formativus had the effect of reproducing always the same type, but it could accomplish its function only if the physical environment remained constant. When the environment underwent some change, the *nisus formativus* suffered a certain degree of deviation. As a consequence, modifications were produced at the embryonic level, similar to these which occurred in monstrosities. Some of these abnormal forms generated in a changing environment in the past could have been viable, thereby constituting the prototypes of new series of beings. Geoffroy's model of saltatory evolution by virtue of such changes led him to object to Lamarck's concept of the tendency to higher levels of organization. He believed that the formation of monstrosities, which he also studied in experiments on the incubation of chickens' eggs, furnished inductive demonstration of the mechanism which was responsible for the succession of beings through past ages. (32) Environmental change and a different state of things in former ages were the prerequisite of Geoffroy's theory of the variability of species “Que la terre, avant qu'elle ait revêtu ses formes actuelles ait été placée sous le régime de milieux atmosphériques et thermométriques différens, et qu'elle ait alors nourri d'autres habitans que les espèces aujourd'hui vivantes, le géologue et le zoologiste sont d'accord sur ce fait.” (33)

Lyell must surely have felt that his fears on reading Lamarck had been made concrete. Because of the eminence in Europe of French natural history, any aggressive theoretical synthesis by some leading naturalist of the tendencies he had noticed would have carried serious consequences. I believe that it was for this reason that he chose to expand the section dedicated to changes in the organic world into the small treatise published as the first part of the second volume of the Principles. He also decided to concentrate on the key point of the transformist system of Lamarck, and, after all, of any transformist approach - the concept of species, and the question of the limits of variability. Our analysis of the chapters Lyell devoted

to this issue will emphasize how his assessment of the problem of species is to be read in the context of his awareness of the tendencies and tensions present in French biological thought of this period.

II

The first topic Lyell dealt with in the second volume of the *Principles* was the question of the variability of species throughout the ages of the earth's history. Lyell was aware that the theory of a succession of types throughout the geological epochs, which claimed a relationship of descent among the various forms of the supposed graduated scale of beings, had now gained some ground in France :

Mr Geoffroy Saint Hilaire has declared his opinion, that there has been an uninterrupted succession in the animal kingdom effected by means of generation, from the earliest ages of the world to the present day ; and that the ancient animals whose remains have been preserved in the strata,

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however different, may nevertheless have been the ancestors of those now in being. (34)

I doubt whether Rudwick was correct in suggesting that Lyell referred here to the *Recherches sur l'organisation des gavials*, which Geoffroy published in 1825. (35) Lyell did not seem to know in detail Geoffroy's theories of mutation by monstrosity, or the structure of his transformist interpretation of the succession of beings, though he had a thorough enough knowledge of Geoffroy's position in general. I rather believe that Lyell is here translating a paragraph of a review of another of Saint Hilaire's memoirs, which he had read in Férussac's *Bulletin des sciences naturelles et de géologie*. One sentence of this review strongly evokes Lyell's abstract of the ideas of the French naturalist :

M Geoffroy Saint Hilaire croit à une succession non interrompue du règne animal, opérée par voie de génération depuis les premières âges du monde jusqu'à nos jours. À la vérité, les antiques animaux dont les debris nous ont été conservés à l'état fossile diffèrent tous, ou de moins presque tous, de ceux qui existent à la surface de la terre. Mais ce n'est pas une raison pour croire qu'il ne puissent avoir été les ancêtres des ceux-là. (36)

At all events, Geoffroy was mentioned here only in order to make it clear that the theory of the succession of species was familiar to authoritative naturalists. The goal of Lyell's polemic was Lamarck himself, whom he rightly identified as the most famous representative of transformism. If the younger generations of naturalists, both in France and - potentially - in England, were tending to accept the basic ideas of Lamarck's transformism, dissociating themselves from the more questionable features of his conceptions, Lyell's intention was to show that there was no empirical basis at all to Lamarck's theory. He maintained that Lamarck's hypothesis was based on imagination ; he was convinced that the French naturalist had begun by considering unsolved problems in the natural sciences, originated by the general crisis of the systems of classification : the increased anatomical knowledge of species ; their present and past geographical distribution ; the numberless forms daily brought to the attention of naturalists ; the links of affinity which seemed to connect species and families to each other. Naturalists certainly felt the need for a theory able to organize this vast range of new information, but Lamarck's solution amounted to an explanatory model which was not supported by concrete and observable facts. It was therefore impossible, according to Lyell, both to accept Lamarck's transformism and to profess a dislike for the actual structure of his thesis. For Lyell, it was methodologically unsound to accept on such terms such a theoretical scheme of interpretation. Somewhat ironically Lyell, in fidelity to his inductive principles, inadvertently acted thus as Lamarck's publicist, producing by far the most compréhensive and accurate summary of Lamarckian ideas produced in England up to that date. (37)

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Lyell went into more detail when he dwelt upon the supposed internal mechanism which, according to Lamarck, produced variations. External circumstances acted indirectly on the organism. The changed

environment obliged, so to speak, the individual to modify its habits, needs and wants. It was therefore a changed need which induced new habits and, eventually, new organs. But Lyell's accuracy and fairness were turned into sharp polemical weapons

We must here interrupt the author's argument, by observing that no positive fact is cited to exemplify the substitution of some *entirely new* sense, faculty or organ, in the room of some other suppressed or useless... We point out to the reader this important chasm in the chain of the evidence, because he might otherwise imagine that we had merely omitted the illustration for the sake of brevity, but the plain truth is, that there were no examples to be found ; ... (38)

Nor did Lyell miss the opportunity of pointing out the cosmogonic character of Lamarck's hypothesis, such as the idea that life had its first manifestations in the depths of a primeval sea. He rightly pointed out the unoriginality of this theory, though this effort of erudition ended in a curious mistake. Instead of attributing the work *Télliamed, ou entretiens d'un philosophe indien avec un missionaire français* … to Benoît de Maillet (1656-1738), he gives the name of Jean Claude de Lamétherie (1743-1817) who was the editor of the *Journal de physique* up to his death in 1817. (39)

However, having attacked Lamarck with sufficient strength, and having shown his great dislike for any theory which led to such a materialistic conclusion, Lyell could afford to concede, in a very careful sentence, that Lamarck's work had a certain interest, to the degree that it attempted to give a naturalistic explanation of some problems in natural history, without recurring to the “repeated intervention of the First Cause”. (40) But even this consideration did not ameliorate the fact that Lamarck's theory was based on unwarrantably long series of assumptions, some of which had unacceptable overtones.

The naturalists of the early nineteenth century were faced with mounting problems in taxonomy. Some saw the hypothesis of transmutability as the way to order ; others, including Lyell himself, expressed deep reservations about its value. Lyell actually argued that the system of classification of the *Animaux sans vertèbres* could restore belief in the fixity of species : Lamarck had been able to establish the specific character of several thousand specimens of fossil shells and of molluscs ; Lyell commented on the paradox that it was Lamarck's work which led the naturalist to form “an exalted conception of the degree of exactness to which specific distinctions are able to be carried, rather than to call in question their reality”. (41) Lyell believed that increased knowledge of numerous tribes of animals and plants would confirm the idea that organic nature is ordained according to some sort of chain of beings, or at least in a highly complex

gradation of forms. Some varieties of a species might therefore be found which differ from one another more than did two related species.

If we consider the multifarious varieties of the dog, the most amazing example of diversification, we are faced with a model demonstration of this phenomenon ; but Lyell insisted that this example was not an instance of diversification still in progress, but only of the great specific capability of the dog to vary in some of its morphological features. Equally, the great difference between domesticated races and their wild ancestors, Lyell argued, did not meet the transformist's requirements, since man had domesticated only highly adaptable species which were able to follow him in his migrations throughout the world. The different degree of adaptability conferred upon each species could become a further criterion for specific distinction, thereby constituting in Lyell's eyes another proof of the fixity of species.

Furthermore, the mummies collected by Geoffroy Saint Hilaire during the Napoleonic expedition to Egypt (1798-1801) presented naturalists with the opportunity of examining animal species which had lived four thousand years ago. The result of the comparison of these remains with living examples of the same species confirmed that after a great lapse of time, no differences were to be perceived between past and present types of the same species. Lyell seemed here to ignore Geoffroy's memoir on the organization

of gavials, in which he questioned the identity of the Egyptian remains with the present-day species. (42) Lyell preferred to follow the established tradition, insisting that there was no perceptible change.

Another phenomenon which had been taken as evidence for the transmutation of species was the modification obtained by horticulturists in their hybridization experiments. Lyell denied that hybrids could be seen as a species in the making ; he repeated Prichard's and Cuvier's views on hybrids, and put the emphasis on the effort which was necessary to produce even the more ordinary forms of hybridization. (43) In the case of plants, it was so difficult to obtain the kind of impregnation needed for the production of a hybrid that it had to be supposed that very few hybrids were produced in a natural state. But even if it were admitted that hybrids occur naturally more often than was supposed, it did not follow that they would be able to produce fertile offspring : “In the universal struggle for existence”, Lyell commented, “the right of the strongest eventually prevails ; and the strength and durability of a race depends mainly on its prolificness, in which hybrids are acknowledged to be deficient.” (44)

Lyell's observations on the struggle for existence, and on the subsequent elimination of weaker plants, closely followed de Candolle's comments about compeunon. (45) It is clear that Lyell applied de Candolle's considerations to the question of hybrids, and brilliantly argued that these

would never survive in a natural environment. Lyell proposed that the degree of difficulty of producing hybrids between two species could be regarded as a further test of natural affinity between species. In any case, the experiments on hybridization could not support Lamarckian theories ; hybrids were generally degenerate forms in respect to the parent species. It was therefore not only inadmissible to maintain that new types are generated in nature by means of hybridization ; it was also clear that such generations would be incapable of producing higher degrees of complexity of organization.

On the theme of variations induced by a changed environment or by domestication, Lyell again followed Prichard's line of thought, and argument that species, when they showed a capacity for adaptation to a new situation could achieve this in a very short space of time : the greatest amount of variation would occur when the new conditions were first encountered, the whole process being accomplished in the course of a few generations.

Prichard's solution was substantiated by two French authorities, Fréderic Cuvier (1773-1838) and Dureau de la Malle (1777-1857) ; Lyell made also some references to François Desiré Roulin (1793-1874.), who wrote an essay on the changes experienced by domesticated types when left free to revert to a savage state in newly colonized regions. The bulk of Lyell's information was derived from Fréderic Cuvier's studies, however, especially those on the social instincts of animals. Fréderic Cuvier wrote two important articles, “De la sociabilité des animaux”, and the “Essai sur la domesticité des mammifères”, in both of which he dealt with some of the basic questions of ethology. The study of the habits of different tribes of animals reflected, in Cuvier's case, an interest wider than the “purely scientific”. The younger Cuvier directly attacked the analogy between the development of organs and of faculties. He observed that the seals kept at the menagerie of the Muséum d'Histoire Naturelle in Paris revealed striking proof of intelligence, despite their being less “developed” than other mammals : “Chez les animaux, pas plus que chez l'homme, l'étendue de l'intelligence n'est pas proportionelle à la perfection des organes.” Anti-transformist polemic pervaded Fréderic Cuvier's essay ; he denied that the process of domestication bestowed a new set of instincts or of habits upon a species. It was indeed possible to develop the instincts of certain species and to make them subservient to man, but only if the species was naturally endowed with a basic set of perfectible instincts. (46)

The same attitude was shared by Dureau de la Malle. A friend and colleague of Desnoyers, he deployed his vast classical knowledge in a series of papers on ancient geography, and on species which

had been domesticated at earlier historical periods. De la Malle's works were referred to by Lyell, albeit often rather cavalierly. For instance, Lyell

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claimed that the domestic qualities, once present in a species, did not require a long period for development, whereas the French scholar believed that domestication was a lengthy process, involving an incalculable potential of perfectibility. (47)

The question of the gradation of intelligence throughout the scale of beings was the last argument touched upon in Lyell's defence of the fixity of species. Lamarck's ideas seemed to have found an indirect confirmation in Friedrich Tiedemann's (1781-1861) studies on the structure of the brain. (48) Lyell could have had access to the 1826 English edition of Tiedemann's work, but it is also possible that he knew about Tiedemann's ideas, which were very popular in Europe, through Antoine Étienne Renaud Augustin Serres's (1786-1868) *Anatomie comparée du cerveau*, which Lyell often quoted, and from which he translated an entire paragraph. Tiedemann maintained that in the course of foetal development the brain passed through fish, reptile and bird stages, before assuming the mammalian form. The possibility of a conjunction between transformism and Tiedemann's theories, constituted a further instance for Lyell of the impending danger of a large-scale revival of Lamarckism, or of some form of it. Lyell followed Serres in recognizing the implications of Tiedemann's ideas. In the paragraph translated by Lyell, Serres concluded his mention of Tiedemann's theories - which, according to him, were a demonstration of the "philosophical spirit" pervading the natural sciences - by remarking that "dans les déformations variées que peuvent éprouver les êtres organisés, jamais ils ne dépassent les limites de leur classe pour revêtir les formes de la classes superieure. Jamais un poisson ne s'élévera aux formes encéphaliques d'un reptile ; celui-ci n'atteindra jamais les oiseaux, un oiseau les mammifères." (49) Again a phenomenon which could be used in a transformist model was turned to prove that the limits of variation, even in the extreme case of monstrosity, are well defined, and no monster could ever reach a superior level in the scale of organization. As in the case of hybridizations, the only effect of monstrosity would be to degenerate the species, not to transform it into a more complex one.

III

The question of the geographical distribution of animals and plants was assuming a crucial role in the debate on species. The laws which regulate the phenomena of the geographical distribution of living beings throughout the world could reveal fundamental insights into the process of species differentiation. Prichard, Lyell, and, as we will see, de Candolle, Bory and Férussac, were convinced that the study of botanical and zoological geography was closely linked with the issue of transformism. Alexander von Humboldt had made geography popular through his universally known voyage to the tropical regions of the New World. Robert Brown, the famous English botanist, had also examined the

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question of geographical distribution in his *Prodromus florae Novae Hollandiae.* (50) Finally, Lyell's friend, Fleming, had dwelt upon the geographical distribution of animals in his *Philosophy of zoology*.

But the most eminent figure of the time - the father, so to speak, of studies of the geographical distribution of plants - was undoubtedly Augustine-Pyramus de Candolle (1778-1841). Lyell knew de Candolle quite well. The naturalist presented to him a copy of his *Essai élémentaire de géographie botanique*, and discussed with Lyell questions relating to geographical distribution. (51) De Candolle probably played a determining role in reinforcing Lyell's belief in the fixity of species. In the *Essai*, de Candolle wrote :

Toute la theorie de la géographie botanique repose sur l'idée que l'on se fait de l'origine des êtres organisés et de la permanence des espèces. Je n'entreprendrai point de discuter ici ces deux questions fondamentales, peut-être insolubles ; mais je ne puis me dispenser de faire remarquer les rapports avec l'étude de la distribution des végétaux. Tout l'article qu'on vient de lire est rédigé en suivant l'opinion que les espèces des êtres organisés sont permanentes, et que tout individu vivant provient d'un autre être semblable à lui ; j'ai cherché à montrer qu'en suivant cette opinion, à laquelle tous les faits certains nous conduisent, et qu'on n'attaque qu'en combinant les conséquences de faits douteux ou ambigus, on pouvait se rendre raison de la plus grande partie de la géographie des plantes. (52)

Lyell's survey of the major aspects of geographical distribution led him to consider what hypothesis could explain all the observed facts. He excluded Linnaeus's theory of special centre of creation, and proposed his own hypothesis : “Each species may have had its origin in a single pair, or individual, when an individual was sufficient, and species may have been created in succession at such times, and in such places as to enable them to multiply and to endure for an appointed period, and occupy an appointed space on the globe.” (53)

Lyell's hypothesis could explain how isolated locations, or islands in the middle of the sea, had a characteristic fauna and flora. Furthermore, he intended to dissociate himself from some tendencies appearing among some supporters of the theory of ***foci***, or centres of creation, the theory that had been proposed in opposition to the hypothesis of a single centre of creation. The polycentric theory had a considerable influence amongst students of geographical distribution. De Candolle mentioned in his *Essai* Karl Ludwig Willdenow's (1765-1812) idea that mountains were “le centre premièrement peuplés par les végétaux”. (54) Antoine Desmoulins (1796-1828), the unorthodox disciple of Cuvier, wrote a *Mémoire sur la distribution géographique des animaux vertébrés* in which, following Louis François Ramond de Carbonnières’ (1755-1827) *Observations faites sur les Pyrenées* (1789), he declared that the hypothesis of distinct centres of creation was the only one able to explain the complexities of geographical distribution. Amongst Lyell's French friends, the baron de Férussac,

whose work on the geographical distribution of invertebrates Lyell quoted, had proposed a version of the theory of the centres of creation, according to which “des bassins de création” should be regarded as analogous to “des bassins et des massifs hydrographiques” in the field of physical geography. (55)

In the article “Création” of the *Dictionnaire classique*, to which Férussac made explicit reference, Bory de Saint Vincent had proposed his transformist version of the theory. Bory began by considering different instances of new creations produced by the modification of species. If a species could vary by the action of external circumstances, then it followed that the “puissance créatrice” is continuously increasing and modifying its works. The intestinal worms, for instance, were undoubtedly new creations. But a more impressive instance was the numerous families and genera which are found in newly formed islands. The island of Mascaraigne, created by the eruption of a volcano, is not far distant from Madagascar. Bory visited the island and described the flora ; he examined all the possible means of dispersion which could have been responsible for peopling the island. But as he remarked, some of the species living there “sont exclusivement propre au pays qu'on ne revoit nulle part, et qui, par conséquent, n'ont dû être crée que sur les lieux mêmes… Il faut nécessairement admettre la possibilité de Créations modernes, de Créations actuelles, et mêmes de Créations futures”. (56)

Bory's theories were summarized approvingly by the authoritative *Edinburgh rev*iew, in a paper written by James Wilson (1795-1856). The author underlined the difficulties in explaining geographical distribution ; he also emphasized the relationship which existed between the question of distribution and the problem of the “origins of beings”, and remarked that :

As it is a matter of certainty that many of these islands are of more recent origin than the great continents of the earth, some recent speculators have argued from this the necessity of admitting the possibility of a comparatively modern creation of animal and vegetable life, whenever such a concurrence of favourable circumstances has taken place in any particular point of

our planet, as determines the completion of those wondrous plans which an all-wise and ever provident Ruler has seen fit previously to organize. (57)

An impartial footnote directed the reader requiring further information to the article "Création" by Bory. It could therefore be argued that Lyell, after stating his hypothesis of the creation of species in succession, and in different places, intended to make it clear that no similarity existed between his theory and the more radical developments in the theory of centres of creation. He emphasized that his hypothesis enabled the naturalist to explain the peculiarities of the flora and fauna of islands, and more generally of isolated localities.

"Now, this congregating, in a small space, of many peculiar species, could give an appearance of *centres* or *foci* of creation, as they have been termed, as if there were favourite points where the creative energy has

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been in greater action than in others, and where the number of peculiar organic beings have consequently become more considerable".(58) Lyell did not intend to "call in question" the theory of centres of dispersion of species. What he objected to was the inference that "a greater exertion of creative power" had taken place at those locations.

It was, however, necessary to avoid the opposite danger of identifying his theory with the catastrophist interpretation of Cuvier's theory of "révolutions". According to Cuvier, great revolutions had succeeded each other throughout the ages, and have caused the disappearance of the fauna and flora living at each period in the localities affected by each "boulversement". Although Cuvier did not adopt an extreme formulation of the catastrophist model, numerous popularizations, and even works by certain geologists, proposed that alter any "catastrophe" an extraordinary exertion of God's creative power restored life upon the earth. (59) Lyell attacked the catastrophist and creationist overtones in the popularizations of Cuvier's theories, commenting on the solution proposed to the problem of disappearance of species by the famous Italian geologist and naturalist, Giovan Battista Brocchi. Brocchi had made clear his dislike for any extraordinary exertion of supernatural agents. He maintained that each species had probably been endowed with a certain determined span of life, and that species, like individuals, died as if affected by old age. (60) Grant himself took note of Brocchi's theory. In the paper in the *Edinburgh new philosophical journal* already mentioned, he dwelt briefly upon the problem of extinction, and referred to Brocchi's theory as a hypothesis worthy of consideration, though he of course preferred Lamarck's solution. (61)

From his own point of view, Lyell believed that Brocchi's theory was another result of incorrect methodology in the natural sciences. Extinction was clearly demonstrated by the geological remains. Some authors developed a theory based on imagined catastrophes ; others, like Brocchi, attempted a different solution, but lacked a correct methodology of research. Lyell set out to prove that his own form of uniformitarianism was methodologically sound, and would lead the naturalist towards a correct solution of the problem. Lyell proposed to approach the question of extinction according to the method described in the first volume of the *Principles*. The naturalist should ascertain what causes were currently in operation on the surface of the earth, determine what power of modification they had, and what consequences were deducible from their coordinated action. The naturalist "must examine attentively the circumstances which determine the *stations* of particular animals and plants" and all the processes which can modify the station themselves, from both the physical and the organic point of view. A detailed and documented survey the "balance of nature" argument, and of the ecological mechanisms which alter and restore the equilibrium in animal and vegetable populations, led to the conclusion that "the successive destruction of

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species must [now] be part of the regular and constant order of Nature". (62) Lyell had therefore reached a highly satisfactory conclusion without recourse to cosmological hypothesis, or to imaginary tendencies, while avoiding any alliance with materialistic lines of thought. He was able to prove that the extinction of species was a natural process, brought about by the operation of natural causes constantly in action.

Conditions within a given geographical region, or even within a limited zone, were kept in a steady state by the agency of numerous factors - organic and inorganic - which produced the "balance of nature". But a modification, though slight, suffered by a single element responsible for the conditions of equilibrium could have a chain of effects on beings which were directly or indirectly related to the agent which had been modified. Man himself, for instance, by eliminating some dangerous species, had caused the extinction of several animal tribes. Fleming had touched upon this subject in a paper published in the *Edinburgh new philosophical journal*, where he showed how the progress of civilization had induced the disappearance of several species formerly inhabiting the British Isles. (63) There were not only instances of animals which had disappeared as a consequence of the development of human society in the first period of human life on earth ; a striking example had occurred in recent times of the complete extinction of a species, the Dodo, or Dronte, which had been firmly established in its station, when the first Dutch explorer visited the island of Mauritius. In a few decades the animal completely disappeared, so that some naturalists even doubted whether it had ever existed. (64) To those who objected that the extinction of the Dodo was brought about by the action of man, Lyell repeated that the action of man did not constitute an exception in the economy of nature. The contemplation of the causes of change in terms of population dynamics, Lyell concluded, allowed the inference that their continuous action would modify the state of organic creation in the course of ages. (65)

This example was chosen in order to show the working of his model, and was designed to prove the validity of his theory, and its superiority over Lamarckism and every possible revival of Lamarckism. Lyell considered the case where species were limited in their territorial expansion by the unfavourable temperature of surrounding localities. Lyell applied to this example the approach followed by de Candolle in the case of plants living in different soils, and remarked that with a slight change of climate, and a lower temperature in a zone, the species favoured by a higher temperature would be weakened, and, might be destroyed by a neighbouring species which could easily tolerate a lower temperature. A species would never have been allowed to change and modify itself according to the changed condition, but would have been eliminated by competing forms. Lyell's model was therefore designed to provide a definitive refutation of Lamarckism. (66)

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It was now clear that this model could have explained the supposed anomalies in the geographical distribution of animals and plants, such as the presence of very peculiar structures in isolated localities and islands. Such species were to be regarded either as very recent ones, which had had not time to spread, or as very old ones, which were survivors of an ancestral, population. However, if the extinction of a species was established as a natural phenomenon, the mode of operation of which was observable in every natural station, another fundamental problem still remained unsolved : "is it possible that new species can be called into being from time to time, and yet that so astonishing a phenomenon can escape the observation of naturaliste ?" (67) Indeed, no one had ever observed such an astonishing event, and it was clear that Lyell's method of taking into account the agents at present in operation, in order to infer by analogy their action in the past, was not applicable to the problem of the appearance of new species. Lyell felt the need to explain why it was not possible either to observe the production of new species, or propound a mechanism, real or theoretical, for such an event. He embarked therefore on a very sophisticated order of considerations, closely resembling his friend Charles Babbage's favourite kind of calculations, and concluded that it would "require more than eight thousand years" before a new species could be produced, or an old one extirpated, in a region of the extent of Europe, and within the class of mammals alone. Thus, no satisfactory answer could be given to the question of the production of new species ; Lyell was trapped in his balance of nature model, which had no power to allow newcomers to

settle, and indeed took great care in eliminating them, either as invaders or as hybrids. Is, therefore, the introduction of new species a natural process, carried on by natural causes ?

we have some data to guide the conjecture, and to enable us to speculate with advantage, but it would be premature to anticipate such discussions, until we have laid before the reader an ample body of materials amassed by the industry of modern geologists. (68)

No such discussions followed ; Lyell did return to this ambiguous conclusion in the third volume of the *Principles*, where he more explicitly declared that the appearance of new species is part of the ordinary course of nature, and again in some letters he later wrote to J. F. W. Herschel and to W. Whewell. (69) Lyell's discussions of the species question had dealt comprehensively with Lamarckism, but had also reinforced the legitimacy of asking the still crucial question : if the introduction of a new species is part of the scheme of nature, how does it actually happen ? With the different solutions given to this question, we enter fully into the debates or species which animated the following decades of the century. As far as Lyell's contribution to the general discussion of the problem of species, and the wider dimension of biological debates in the 1820s and early 1830s

are concerned, my analysis of Lyell's second volume has shown how good a witness of his time he was.

Historians have too readily assumed, under the influence of Cuvier and Marie Jean Pierre Flourens, that Lamarck's theories had no followers, provided no inspiration, and were considered by the overall majority of French naturalists as disreputable and unscientific. The debate between Cuvier and Geoffroy Saint Hilaire has therefore been regarded as an attempt by the victorious Cuvier to silence his opponent. I would like to suggest that it was in fact Cuvier who was under some pressure during the 1820s. (70) Cuvier's policy of remaining silent, whenever possible, on the various transformist theories currently advanced, and on the continuous ascendancy of the French brand of transcendental anatomy, had to be changed in the rate 1820s, when the idea of a unity of plan pervading the whole of the animate creation found many and authoritative supporters, and was looked on with positive interest even by naturalists who, like Serres, shared many of Cuvier's ideas on the methods and goals of natural history. Cuvier decided then to take the first good occasion to launch his counter attack.

Lyell showed in his *Principles* that he was aware that it was possible to graft some of the tenets of the transcendental anatomists on to some modified form of transformism : indeed, Geoffroy was authoritative enough to make the possibility of a large-scale revival of a new form of transformism very possible. Serres himself, as we have seen above, was not unaware of the possible transformist application of his own theories. Furthermore, if Georges Cuvier avoided commenting on the theories of his adversaries as much as possible, his younger brother Fréderic did express much concern for the materialistic implications of a theory of development of mental faculties, which were seen as linked to the process of complication of organization in the nervous system.

For several reasons, Lyell preferred to concentrate on the work of Lamarck himself. Obviously, Lamarck was the most famous representative of transformism, but at a more sophisticated level of cultural polemic, Lyell probably wanted to emphasize that any coherent form of transformism was to be considered as a modification of Lamarckism, and therefore as a theory based on imagination. Lyell also believed, together with his friend Fleming, that a revival of transformism, or of some form of Lamarckism, was a possibility not limited only to France. He saw signs of the diffusion of transformism in England itself, where it could even form an unholy alliance with prevailing progressionist and directionalist interpretations of the history of life on earth. If, therefore, the biological debates in France in the late 1830s saw a retreat of transformist theories, their defeat had not been beyond dispute in the 1820s and early 1830s.

I hope I have been able to point out the necessity of taking into systematic account channels of diffusion of ideas other than major

published works. The *Bulletin des sciences naturelles*, and the *Dictionnaire classique des sciences naturelles* are only two of a far greater number of sources which are still waiting thorough and sympathetic examination. A figure like Bory de Saint Vincent, with his interesting attempt to revive eighteenth-century materialistic ideas within the context of early nineteenth-century debates, surely deserves more than the practically negligible consideration he has so far received.

Our understanding of Lyell's position in this crucial period of the history of biological ideas would be greatly improved through an exhaustive attempt to examine not just those lines of thought which eventually triumphed, but how different theoretical and philosophical options were strenuously discussed, and how the eventual rejection of any form of transformism, even if only for a few decades, was not the inevitable and undisputed termination of an often dramatic debate.

NOTES

(1) For recent works on Chartes Lyell see : M. J. Bartholomew, "Lyell and evolution : an account of Lyell's response to the prospect of an evolutionary ancestry for man", *The British journal for the history of science*, 1973, *6*, 261-303 ; "Lyell Centenary issue" of *The British journal for the history of science*, 1976, *9*, and in particular R. Porter, "Charles Lyell and the principles of the history of geology", pp. 91-103, and M. J. S. Rudwick, "Chartes Lyell speaks in the lecture theatre", pp. 147-55. Amongst other récent publications by Professor Rudwick, see : "Poulett Scrope on the volcanoes of Auvergne. Lyellian time and political economy", ibid., 1974, *8*, 205-42 ; "Charles Lyell, FRS (1797-1875), and his London lectures on geology", *Notes and records of the Royal Society of London*, 1975, *29*, 231-63.

(2) M. J. S. Rudwick, "The strategy of Lyell's *Principles of geology*", *Isis*, 1970, *61*, 4-33. For discussions of the French natural sciences in the early nineteenth century, sec the works cited in notes 4, 49, 59 below.

(3) Charles Lyell, *Principles of geology*, 3 vols., London, 1830-33, iii, XIV. Hereafter cited as *Principles.*

(4) On Lyell's journeys to Italy and France, see L. G. Wilson, *Charles Lyell. The years to 1841, the revolution in geology*, New Haven, 1972, *passim* ; and "The intellectual background to Chartes Lyell's Principles of geology, 1830-1833", in Cecil J. Schneer, ed., *Towards a history of geology*, Cambridge, Mass., 1969, 426-33.

(5) J. B. P. A. Monet de Lamarck, *Philosophie zoologique, ou exposition des considérations relatives d l'histoire des animaux*, Paris, 1809 ; *Histoire des animaux sans vertèbres*, Paris, 1815-22. See also : M. Landrieu, *Lamarck, le fondateur du transformisme, sa vie, son oeuvre*, Paris, 1909 ; H. Daudin, *Cuvier et Lamarck : les classes zoologiques et l'idée de série animale* (1790-1830), 2 vols., Paris, 1926. For reviews of recent themes in Lamarckian studies, sec R. W. Burkhardt, "Lamarck and the politic of science". *Journal of the history of biology*, 1970, *3*, 270-98, and "The inspiration of Lamarck's belief in evolution", ibid., 1972, *5*, 13-38 ; E. Mayr, "Lamarck revisited", ibid., 1972, *5*, 55-94 ; R. W. Burkhardt, *The spirit of system : Lamarck and evolutionary biology*, Cambridge, Mass., 1977 ; L. J. Jordanova, *The natural philosophy of Lamarck in his hisiorical context*, University of Cambridge PhD thesis, 1976.

(6) John Fleming, *The philosophy of zoology*, London, 1822. i, 7

(7) Ibid., p. 14.

(8) Ibid., pp. 311-12.

(9) [J. Fleming], review of J. E. Bicheno's *On systems and methods in natural history*, in *The quarterly review*, 1829, *41*, 302-28 (321).

(10) W. S. MacLeay, "On the dying struggle of the dichotomous system", *Philosophical magazine*, 1830, *44*, 137.

(11) [R. E. Grant], "Observations on the nature and importance of geology", *Edinburgh new philosophical journal*, 1826, *1*, 297. For attribution, see *Dictionary of national biography*, at "Grant, R. E.".

(12) On Fleming's habit of discussion with “young Lyell”, see J. Fleming, *The lithology of Edinburgh*, edited with a memoir by the Rev. John Duns, Edinburgh, 1859, p. lvi.

(13) [C. Lyell]. “Transactions of the Geological Society of London”, *The quarterly review*, 1826, *34*, 507-40.

(14) Hooykaas, “Geological uniformitarianism and evolution”, *Archives internationales d'histoire des sciences*, 1966, *19*, 17 ; M. J. S. Rudwick, op. cit. (2), p. 26.

(15) M. J. Bartholomew, op. cit. (1), pp. 272-6 and *passim*.

(16) [Mrs] K. M. Lyell (ed.), *Life, letters and journals of Sir Charles Lyell, Bart.*, 2 vols., London, 1881, ii, 365.

(17) Sir H. Davy, *Consolation in travel, or, the last days of a philosopher*, London, 1830, p. 150.

(18) Ibid., p. 219.

(19) J. C. Prichard, *Researches into the physical history of mankind*, 2nd edn., London 1826, i, 97 ; quoted by Lyell, *Principles*, ii, 52. For a brief but substantially correct assessment of Prichard's position in the anthropological debates of the early nineteenth century, sec W. F. Bynum, “The great chain of being”, *History of science*, 1975, *13*, 13 and *passim*. For a more complete consideration of Lyell's debts to Prichard's work, see F. N. Egerton, “Studies in animal population from Lamarck to Darwin”, *Journal of the history of biology*, 1968, *1*, 225-59.

(20) J. P. Desnoyers, “Observations sur un ensemble de dépôts marins plus récens que les terres tertiaires des bassins de la Seine, et constituant une formation géologique distincte ; précédés d'un aperçu de la non simultanéité des bassins tertiaires”, *Annales des sciences naturelles*, 1829, *16*, 171-208, 402-91.

(21) G. P. Deshayes, “Conchyliologie”, *Dictionnaire classique des sciences naturelles* (hereafter *Dictionnaire*), Paris, 1823, iv, 377 ; idem, “Mollusques”, ibid., 1827, xi, 42.

(22) Loc. cit. (16), i, 308.

(23) *Histoire des mollusques terrestres et fluviatiles*, Paris, 1838-1851.

(24) Loc. Cit. (16), i, 139.

(25) A. de Férussac, “Monographie des espèces vivantes et fossiles du genre Mélanopside”, *Mémoires de la Société d'histoire naturelle de Paris*, 1823, *1*, 131 ; see also *Bulletin universel*, 1823, *3*, 39-40

(26) Bory de Saint Vincent, *Dictionnaire*, 1825, viii.

(27) Bory de Saint Vincent, ibid. : “Histoire Naturelle”, 1825, viii ; “Matière”, 1826, x ; “Création”, 1824, v ; “Géographie”, 1825, vii.

(28) Bory de Saint Vincent, “Histoire Naturelle”, ibid., 1825, viii, 251.

(29) Bory de Saint Vincent, “Matière”, ibid., 1826, x, 249.

(30) Bory de Saint Vincent, “Instinct”, ibid., 1825, viii, 587.

(31) E. Geoffroy saint Hilaire, “Mémoire où l'on se propose de rechercher dans quels rapports de structure organique et de parenté sont entre eux les animaux des âges historiques, et vivantes actuellement, et les espèces antédiluviennes et perdues”, *Mémoires du Muséum d'histoire naturelle*, 1828, *17*, 209-29 ; see in particular p. 213.

(32) E. Geoffroy Saint Hilaire, “Sur les organes sexuelles et sur tes produits de génération des poulets dont on a souspendu la ponte, en fermant l'oviducte”, ibid., 1822, *9*, 1-24.

(33) E. Geoffroy Saint Hilaire et Marcel de Serres, “Rapport fait à l'Académie Royale des Sciences sur un mémoire de M. Roulin, ayant pour titre : Sur quelques changemens observés dans les animaux domestiques transportés de l'ancien monde dans le nouveau continent”, ibid., 1828, *17*, 207.

(34) C. Lyell, *Principles*, ii, 2.

(35) M. J. S. Rudwick, op. cit. (2), p. 18, note 46.

(36) *Bulletin des sciences naturelles et de géologie*, 1830, *20*, 144-7 (144). This paper is a review of E. Geoffroy Saint Hilaire's work cited in note 31.

(37) For various and different interpretations of Lyell's second volume, see M. J. Bartholomew, op. Cit. (1), P. 278.

(38) C. Lyell, *Principles*, ii, 8.

(39) Ibid., ii, 11.

(40) Ibid., ii, 18.

(41) Ibid., ii, 22-3.

(42) E. Geoffroy Saint Hilaire, "Recherches sur l'organisation des gavials, sur leurs affinités naturelles…" *Mémoires du Muséum d'histoire naturelle*, 1825, *12*, 154.

(43) G. Cuvier, *Discours sur des révolutions de la surface du globe, et sur les changemens qu'elles ont produits dans le règne animal*, 3rd edn., Paris, 1825, p. 121. Cf. J. C. Prichard, op. cit. (44 ; ist edn., 1813), p. 9.

(44) C. Lyell, *Principles*, ii, 55-6-

(45) A. P. de Candolle, "Essai élémentaire de géographie botanique", in F. Cuvier, ed., *Dictionnaire des sciences naturelles*, Paris. 1819, xviii. The quotations will be from an undated offprint copy given by de Candolle to the Tuscan naturalist Ottaviano Targioni Tozzetti (1755-1826), now preserved in the Biblioteca Nazionale of Florence. On the theme of competition in nature, see in particular p. 26.

(46) F. Cuvier, "Essai sur la domesticité des mammifères, précédé de considérations sur les divers états des animaux, dans lesquels il nous est possible d'étudier leurs actions", *Mémoires du Muséum d'histoire naturelle*, 1825, *13*, 415-16, 448.

(47) A. Dureau de la Malle, "De l'influence de la domesticité sur les animaux depuis le commencement des temps historiques jusqu'à nos jours". *Annales des sciences naturelles*, 1830, *21*, 52-3.

(48) F. Tiedemann, *Anatomie und Bildungsgeschichte des Gehirns im Foetus des Menschen nebst einen vergleichenden Darsiellung des Ilirnbaucs in den Thieren*, Nürnberg, 1816. The French edition was published in Paris in 1823 ; the English one, which was a translation from the French, appeared m Edinburgh in 1826.

(49) A. E. R. A. Serres, *Anatomie comparée du cerveau, dans les quatres classes des animaux vertébrés, appliquée à la physiologie et à la pathologie du système nerveux*, Paris, 1826, ii, LXIII. Cf. C. Lyell, *Principles*, ii, 62. For a still satisfactory account of Serres's work and ideas, see E. S. Russell, *Form and function. A contribution to the history of animal morphology*, London, 1916, *passim*. T. Cahn, *La vie et l'oeuvre d'Étienne Geoffroy Saint Hilaire*, Paris, 1962, fails to distinguish between Marcel de Serres and A. E. R. A. Serres.

(50) A. de Humboldt, *Voyage aux régions equinoxiales du nouveau continent*, fait en 1799-1804 Paris, 1814 ; idem, *Essai sur la géographie des plantes, accompagné d'un tableau physique des régions équinoctiales*, Paris, 1807 ; R. Brown, *Prodromus florae Novae Hollandiae et insulae Van Diemen, exhibens characteres plantarum quae annis 1802-1805 collegit et descripsit R. Brown*, London, 1810. See F. N. Egerton, op. cit. (19), and idem, "Humboldt, Darwin and population". *Journal of the history of biology*, 1970, *3*, 325-60

(51) Loc. cit. (16), i, 245 : "He [de Candolle] is in full force, and bas been most useful to me, having given me what cannot be bought, his splendid Essay on Geographical Botany... I have just had this morning a famous geologico-botanical discussion with Prof. De Candolle."

(52) A. P. de Candolle, op. cit. (45) P. 59.

(53) C. Lyell, *Principles*, ii, 124 ; cf. J. C. Prichard, op. cit. (19), pp. 9-l0, 16-18.

(54) A. P. de Candolle, op. cit. (45), p. 58.

(55) A. Desmoulins, “Mémoire sur ta distribution géographique des animaux, moins les oiseaux”, *Bulletin général et universel*, 1823, p. 381 ; L. Ramond, *Obervations faites dans les Pyrenées, pour servir de suite à des observations sur les Alpes*, Paris, 1789, and idem, *Voyage au Mont Perdu, et dans la partie adjacente des Hautes Pyrenées*, Paris, 1801. This last work was quoted at length by Bory de Saint Vincent in his article “Montagnes”, *Dictionnaire*, 1826, xi. Cf. A. de Férussac, “Géographie”. ibid., 1825, vii, 267.

(56) Bory de Saint Vincent, “Création”, ibid., 1825, v, 41, 44.

(57) J. Wilson, review of [?], *Fauna borealis Americana, or the zoology of the northern parts of British America*, Edinburgh review, 1831, 53, 338-40-

(58) C. Lyell, *Principles*, ii, 126.

(59) On the question of Cuvier's “catastrophism”, and its various interpretations, see : E. S. Russell, op. cit. (49) ; M. J. S. Rudwick, *The meaning of fossils*, London, 1972, 134-5, and *passim* ; W. Coleman, George Cuvier, zoologist. A study in the history of evolution theory, Cambridge, Mass., 1964, *passim* ; cf. also F. Bourdier, “Geoffroy Saint-Hilaire vs. Cuvier”, in Cecil J. Scheer, ed., op. cit. (4), pp. 39-61 ; see in particular pp. 43-4. For an early recognition of the misrepresentation of Cuvier's ideas in England, sec R. Knox, *Great artists and great anatomists : a biographical and philosophical study*, London, 1852, pp. 26-9, 42-5.

(60) G. B. Brocchi, *Conchologia fossile subapennina, con osservazioni geologiche sugli Appennini e sul suolo adjacente*, Milano, 1814, pp. 227-9.

(61) R. E. Grant, op. cit. (11), p. 298.

(62) C. Lyell, *Principles*, ii, 141.

(63) J. Fleming, “Remarks illustrative of the influence of society on the distribution of British animals”, *Edinburgh new philosophical journal*, 1824, *11*, 287-305.

(64) For some remarks on the dodo, see G. B. Brocchi, op. cit. (62), p. 236 ; G. Cuvier, “Notes sur quelques ossemens qui paraissent appartenir au dronte, espèce d'oiseau perdue seulement depuis deux siècles”, *Bulletin des science naturelles*, 1830, *17*, 122-5 ; Bory de Saint Vincent, “Dodo”, *Dictionnaire*, 1823, iii ; J. S. Duncan, “A summary review of the authorities on which naturalists are justified in believing that the Dodus, Dodus ineptus, *Linn*, was a bird existing in the isle of France or the neighbouring islands, mail a recent period”, *Zoological journal*, 1828, *3*, 554-66.

(65) C. Lyell, *Principles*, ii, 156-7.

(66) A. p. de Candolle, op. cit. (45), p. 27 ; C. Lyell, *Principles*, ii, 174-5.

(67) C. Lyell, ibid., 179.

(68) G, Lyell, ibid., 183-4.

(69) C. Lyell, *Principles*. iii, 30 ; and loc. cit. (16), i, 464-9 ; ii, 2-5.

(70) See E. S. Russell, op. cit. (49). p. 63 ; but cf. p. 128.